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CHANCE

MY purpose is to show that chance is an objective category; objective, that is, in the same sense as causation, space, quantity, or other accepted scientific categories. By a chance-event, I mean an event which has no cause; though a fuller definition will appear in the course of the argument. The question of the ultimate metaphysical status of the category will not be discussed.

That there are aspects of the physical world which are, in a sense, outside the pale of law and causation, is widely admitted among philosophers to-day. Professor Royce has shown[1] that the element of significance or value which resides in individual things can not be scientifically accounted for; Mr. C. S. Peirce has argued[2] for an ultimate indeterminism out of which grows a certain amount of law; James[3] and Bergson[4] have defended an irreducible spontaneity in all real events; Professor Palmer has lately advocated chance-combinations of causal series;[5] Cournot[6] and others in France have stood for a similar view. Admitting in general the truth of these positions, I wish to carry the argument somewhat further, to give the concept a more positive interpretation, and to place it firmly within the field of scientific categories. Not only is chance, as I believe, more than a mere name for our ignorance; not only is there a certain aspect of fact which is outside of causality; there is a perfectly definable, intelligible tendency in physical events toward variation from law, and this tendency is nearly, if not quite, as widely verified as laws themselves. I shall venture, then, to differ from most of the previous views in regarding chance as a well-defined and, in one sense, a positive category *within* the scientific field or world of description.

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[1] "Spirit of Modern Philosophy," Lecture XII.

[2] *Monist*, January, 1891, April, 1892; also, incidentally, in October, 1892, January, 1893, and July, 1893.

[3] "Some Problems in Philosophy," Chapter IV.

[4] Principally in "L'évolution créatrice." As this is one of the main contentions of the whole book, specific reference is perhaps not needed.

[5] In "The Problem of Freedom," Chapter X.

[6] "Essai sur les fondements de nos connaissances." Many articles on the subject by others have appeared in the *Revue Philosophique*.

Before we go to the evidence for this view, a word must be said as regards the subject of this investigation. It is not the pure or mathematical concept of chance which is here studied, but the empirical; and a failure to distinguish these might lead to misconception or misdirected refutation. The philosophy of scientific categories, toward which this paper aims to contribute, may proceed in either of two ways. It may study such categories *in abstracto,* as pure concepts and members of an ideal system of concepts, without direct concern as to their mode of application to experience; or it may study them, not as members of an ideal system of knowledge, but as their nature is revealed in actual scientific treatment of the facts to which they apply. The former method treats categories as instruments of exact knowledge and perfect determination, a purposive rearrangement of data, due entirely to the activity of mind, and dominated by its ideal purposes; the latter treats them as adaptations, rather, in which the ideals of the mind are less dominant and the intelligence of the knower is more subjected to the data. Examples of the former are the many recent works upon exact logic; of the latter, Bergson's definition of consciousness in "Matière et Mémoire," Dewey's definition of truth in "Studies in Logical Theory," Montague's definition of consciousness in the paper "Consciousness a Form of Energy."[7] In general the results of these methods will not agree, because they study different concepts. Causation as a factor in an ideal system of knowledge may be a very different thing from the causation that is used in the science of to-day. But to the philosopher both should be at least interesting. As to the question, which one is the ultimately correct category, that lies beyond the province of this paper.[8] I consciously choose the empirical concept of chance, seeking to know what, if anything, of the fortuitous is implied in the scientific methods and results of our time.

If we consider the world in cross-section, at one moment we seem to find many causes acting, which themselves bear little if any causal relation to one another. That I am at this moment speaking can not be causally explained, so far as we know, by the fact that the tide is just now turning in the harbor. That the tile on the roof is loosened by the wind and falls just at the moment I pass beneath it (to use the familiar example) may very well be fully determined by antecedent causes; that I pass at that moment may be equally determined; but each of these series seems to be quite undetermined by the other. Here appears a loophole through which chance might enter the scientific realm; many thinkers have been so persuaded. And yet who knows that further scientific evidence might not show the two events related as the scales of a balance? It might be a case of Kantian reciprocity. My passing beneath the house jars the earth, the house, and the tile, however slightly; and if the tide did not turn just now when I speak, something would be wrong with the moon or sun, and who knows what meteorological conditions might immediately transpire, even to the destruction of all of us? It seems to me that we must await evidence on this point. Meanwhile I find nothing in the observed results to rule out a mutual determination of all these facts.

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[7] In "Essays Philosophical and Psychological in Honor of William James."

[8] The clearest statement I have found, of the ideal or conceptual method, is in Professor Royce's "William James and Other Essays," pages 234 ff.

But there is another way in which events might be uncaused. We might consider, not a cross-section of the world at one moment, but a sequence. It is conceivable, whether credible or not, that in a series like the successive positions of a falling body slight variations from the straight path might occur, which were not caused by anything in the past history of that body or any other fact, past or present. Is there any evidence, in present scientific methods or results, of such phenomena? Is there any direct and positive implication of *uncaused* variations from exact law in any of the sequences of this world?

At the present date we have, thanks to the accurate measurements and tabulations of anthropologists, biologists, economists, meteorologists, and others who employ statistical methods, an enormous body of facts of the sort we are seeking. It has been found, for example, that the height of men and women, the length of various organs, the fluctuations of the thermometer, of rainfall, of prices, and so on, show a variation about a more or less ideal type or average. And what is more, the manner of variation is much the same throughout. To quote Professor Pearson: "From paupers to cricket scores, from school-board classes to ox-eye daisies, from crustacea to birth-rates, we find almost universally the same laws of frequency."[9] Nor is such variation confined to phenomena of living organisms. Besides meteorological facts already mentioned, we find that the exactest measurements in our physical laboratories show similar variations in the facts there recorded.[10] But with the exact delimitation of the field wherein such variations occur we are not concerned; enough that they are widely prevalent.

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[9] "Chances of Death and Other Essays," page 20.

[10] Any work on statistics will give an idea of the wide extent of this fact of variation. See, *e. g.*, G. U. Yule, "Introduction to the Theory of Statistics."

We may say summarily that there seems to be a tendency, when experiments are repeated again and again, for the results to vary more or less about an ideal standard, norm, or type. For we may regard each human individual, say, as a repetition of the experiment of producing a human being; each rainstorm as nature's repeated attempt to produce rain, etc. That many such experiments are being conducted simultaneously does not affect the logic of the situation, just as the result is indifferent whether we toss one penny many times or many pennies at once. The examination of large collections, or repetitions of similar phenomena, thus suggests what we could not discover from the single case, namely, that besides the general law which says "be so and so" there is another which says "be not quite so and so." Such at least is the superficial impression we get from the facts. Indeed it seems likely that had the science of statistics been organized as long ago as the other natural sciences, philosophers would scarcely have defended universal causation as frequently as they have done.

But superficial impression is far from demonstration. The mere fact of a wide-spread tendency to vary from a type is hardly the slightest evidence of real chance. Are not all the variations themselves caused? If you are taller than I am, surely there is a reason for it; if to-day's rain is heavier than last week's, atmospheric conditions will account for it. But let us look again at the variations. We said above that their manner was much the same everywhere. And, moreover, that manner is a rather remarkable one. When the numerical values are graphically plotted they reveal a fairly close approximation to the well-known curve of error, or probability-curve. *Exact* correspondence with that curve we do not get, of course; but perhaps no concept, curve, or standard ever fitted the facts exactly. Laws are certainly never exactly fulfilled, yet we accept them. Now this striking unanimity of the variations suggests that they are not completely accounted for, each by its particular causal antecedents, but that a special tendency must be invoked to account for this common property; and that, too, a tendency to vary fortuitously, since the probability-curve is just what would result, in the long run, from fortuitous variation. If a series of murders are committed in a city, or in several cities, with a cross drawn in blood on the forehead of every victim, we should reasonably infer that one man, or band of men, was the author of the crimes. Such coincidence would be the strongest kind of circumstantial evidence. Our case seems just like that. It is hard to resist the conclusion that there is a wide-spread tendency at work in nature, making each event a little different from what it would be if all were governed by absolute law.

But the belief in universal causation, at least within the sphere of science, is so ingrained in us by our modern education that it is not easily dislodged. *Must* this resemblance to the probability-curve be explained by a fortuitous tendency to vary? For, if there is anything less than a strong logical compulsion here, we can hardly abandon that widely attested concept of law. We must then ask whether the facts could not possibly be explained without the resort to chance. And in answer I shall try to show, first, that a special tendency to vary must be begged, and secondly, that this tendency must be such as to permit chance to the individual cases, though not to the group as a whole. No other explanation of the situation, I shall claim, will do justice to the facts.

First, then, can not the resemblance to the probability-curve be explained on the hypothesis of universal causation? Let us see how that hypothesis would work out. Consider the case of the heights of a large number of men in a given city. When their numerical values are plotted, we have an approximation to the said curve. The height of each man is undoubtedly dependent on many causes, such as inheritance, nourishment during years of growth, early health, open air, sunlight, amount of fatigue in early life, etc. Now if you take a great number of men, these causes are certain to vary greatly from man to man. They will combine very differently in the individual men, giving very different results. And if you take men enough, you will include all possible combinations of these many causes. And this is no affair of chance, but is certain to be the case. Every possible effect upon the height of a man will thus be realized, and this, as is well known, will give a result approximating the curve. No special tendency toward variation need be conjured up, therefore; the large number of ways in which the causes affecting growth will combine, guaranteed by the large number of men measured, will suffice to account for the facts. So much for the hypothesis of causation. As a matter of fact, it seems to be the view of many writers on the subject.[11] Yet I can not but regard it as unsatisfactory. That each variation is indeed due to many cooperating causes is indisputable. That it can be *wholly* explained by those causes is a very different matter. For it is a condition of the formation of the curve that all possible combinations be realized in equal numbers. And there is nothing in the causal explanation to ensure this. The mere fact that by taking cases over a wide enough area you get all possible combinations of causes will not determine that those different combinations occur in anything like approximately equal numbers. But they do so occur. Accordingly, I think we are driven to say that over and above the known and unknown causal laws there is a special tendency, active in nature, to realize in the long run every possible combination of causes in equal numbers. And since the individuals that vary, whether they be human heights, or organs, or prices, or temperatures, or what not, are themselves the products of many causes, we may perfectly well say that individual phenomena themselves tend to vary equally in all possible directions about a type. I say "in all *possible* directions," for the variation is always, apparently, restricted to a rather narrow field. But within that field, at any rate, a clearly marked and positive tendency, in addition to the usual kinds of causation, seems a necessary hypothesis.

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[11] *E. g.*, Venn, "Logic of Chance," page 475, footnote. Jevons, "Principles of Science," page 196. Laplace, "Philosophical Essay on Probabilities," page 4.

Of course "tendency" is a vague word and renders one liable to the accusation of hypostasising an abstraction. But it is here used as no more than a concept or formula to summarize a large class of facts. Exactly the same is true of such concepts as causation and of the particular causal laws to which we accord our belief. In a sense they explain nothing and solve no mysteries. I do not here claim for the tendency in question any deeper validity than we ascribe to the usual causal laws; but if the argument so far is correct, it should have at least as much validity as those concepts have. We should speak of a real tendency among events to vary about a type, even as we speak of a real tendency in bodies to fall, or a real tendency in heat to radiate.

We come now to the second point mentioned above. May not this tendency to vary be itself a unique kind of a causal law, strictly determined in every detail? If it is so orderly and regular on the whole, must it not be equally so in every particular case? The probability-curve is a very regular affair, and the variations of phenomena are, on the whole, very regular too. We find approximately the same proportion of heights above the mode, the same below, again and again. How could the collection be so orderly if the individual members were lawless? In short, we must now examine the individual instances, to see how this collective tendency should be interpreted in its application to them.

If the tendency to vary is operative through the series as a whole, it can not well be *nil* in any one event. What form, then, must it assume in one such event? There must be a tendency for each event to vary somehow from the norm. And further, it must be either predominant in one direction, or equal in all directions. On the latter alternative, the various directions counterbalance one another, and nothing can decide which variation will occur except some cause external to the event itself, or just chance. But it will not suffice us to appeal to an external cause to decide the matter. For, as we have seen above, the appeal to such causes will not account for the collective character of the variations. It will not guarantee what must be guaranteed, that the variations will, in the long run, be fairly equal in all possible directions. If some particular, external cause decided, in each instance, which of the conflicting directions should prevail, we should not, in general, have in the series as a whole the all-inclusive manner of varying that we do find. The only alternative is chance. This and this only would seem to allow to the series that elasticity which enables each instance so to combine with the others as to give the total result we observe. If then the tendency to vary is in each instance equal in all directions, the actual result in that instance must be ascribed to chance.

But perhaps in each case the tendency to deviate is strongest in one direction, changing in accordance with some fixed and unknown law as the cases are repeated, and gradually covering all possible cases. This again would seem to reduce all to strict causation. To be sure the variations seem to be essentially irregular and disorderly, but that may perhaps be due to our ignorance. May not the tendency to vary be itself found an orderly and thoroughly determined affair if we could only study it carefully enough? To this question I must answer, no. The collective tendency toward *variation* seems to me inconsistent with causal determination of the individual case. It is, I think, generally agreed within the scientific field that one and the same cause can not, under constant conditions, produce varying effects. The cause we are discussing is the *tendency to vary,* which is, perhaps, in *some* sense, one and the same throughout the series. In so far as it is the same it must be supposed to produce, under similar conditions, much the same results. Now the conditions in all the individual cases are, to all intents and purposes, the same throughout. For our tendency acts *independently* of these special circumstances of each case. We have already seen that those circumstances could not guarantee the nearly equal distribution which occurs, and that consequently the tendency in question must be begged; and its action must be the predominant one if the result is to be secured. Each variation might then be treated as if it were due to that tendency alone. But *that* seems to me equivalent to having the conditions constant: the tendency to vary acts as if it were in isolation. It produces, however, as the experiment is repeated, ever-differing results. As this would seem inconsistent with the causal action of the tendency, such action must be denied, and we must say that the individual variations could not possibly be caused by one tendency. Even if we discovered some time a hidden regularity about the variations, an order expressed by some function beyond our present knowledge, that order would have to be regarded as fortuitous. For the fact that there occurred different results from one and the same cause would be, for science, an inexplicable thing. Is it answered: "Perhaps your tendency to vary is not one tendency but a manifold complex of them"? The same inconsistency with causation would, I believe, hold even then. In so far as the complexity obtains, it means, after all, *at bottom,* many independent (*i. e.,* fortuitous) tendencies. In short, no *one* tendency can explain an ever-varying manifold of effects, and *many* tendencies, in so far as they can not be reduced to one, themselves constitute chance. It is the spreading or multitude of the effects that, in my opinion, renders a causal explanation impossible.

In cases of ordinary causation, the same cause does indeed produce ever-varying effects. But that is because it acts in ever-differing circumstances, and its action is influenced by those circumstances. Our tendency however can not, in the long run, be influenced by them. It acts *with* them and *in* them, but it must predominate over them if the equal distribution is to result. And it is this predominance, or causal isolation in a certain sense, which is the key of the situation. The manifoldness of the effects has nothing left to explain it but just its own manifoldness. From one isolated principle you can never get many results, and the many results can not combine into just one isolated principle.

The conclusion thus seems to be forced upon us that our hypothesis of an all-inclusive collective variation implies complete ambiguity in the single case. We have then obtained, if the argument is correct, the following principle: there is a tendency, in many phenomena, to vary with equal frequency in all possible directions from obedience to law, the variation being such as to give regularity for the group as a whole, chance for the individual member. Of course this tendency is hardly ever, if ever, completely realized. It is a limiting concept, like that of law and causation. But it gives what is to my mind a more positive signification to chance than has usually been ascribed to that notion. Not *mere* irregularity, but a tendency to spread, to diverge, so as to treat all possibilities fairly and give them an equal showing—that, somewhat metaphorically expressed, is what I think we should mean by chance. Of course these possibilities are not absolutely infinite in any one case; they are always restricted by the special circumstances of that case. Men probably can not vary much in height; temperatures in a given region range hardly more than a few degrees out of the long scale known to science; and in general the field of chance is relatively small. On the other hand, we seem to find some amount of chance accompanying almost every case of law. How wide the field of variation is, in each class of phenomena, would seem to depend on the nature of the causes whose combination gives rise to the phenomena. But the whole matter is an empirical one. Our view gives no occasion for those caricatures, as Professor James called them, which would accuse its advocates of believing that *anything* might happen in a given situation. Nor does it offer a contradiction to the principle of causality. Each variation is the resultant of many causes together with a chance-deviation. It would not be regarded as a denial of the law of gravitation if I held up a ball in my hand. No more does it deny the constant action of causes to assert that there is another principle cooperating with them. But the view I defend would imply partly uncaused beginnings, arising to some extent *ex nihilo*. Should a last stand be made on the ground that the principle of the conservation of energy would forbid any uncaused changes, we need only remember that the measurements which prove the conservation of energy are themselves subject to the same kind of variation as that we have been exhibiting.

Finally let me indicate the relation of the above view to some previous arguments for and against indeterminism. It is well known that the more we learn about any given event, and the finer our measurements become, so much the closer is the approximation to exact law. The conclusion seems to many thinkers to follow inevitably, that a perfect knowledge, measurement, etc., would reveal perfectly exact law. It seems to be a case of a variable approaching a limit, as a hyperbola approaches its asymptote, or the series $1 + \frac{1}{2} + \frac{1}{4} +$, etc., approaches the number 2. But the mere fact that we get gradually nearer and nearer to exact law does not imply that the latter is the limit we are approaching. If a line be drawn parallel to the asymptote and beyond it, the curve gets nearer and nearer to that line, but does not approach it as a limit; and the series $1 + \frac{1}{2} + \frac{1}{4} +$, etc., gets nearer and nearer to 3 without approaching it as a limit. Such reasoning is then quite inconclusive. Moreover, it overlooks the fact, which is the pivot of my argument, that the deviations from exact law themselves, when recorded and measured, show a positive manner of varying which can hardly be explained by causation. It is in this point that the present argument differs, so far as I know, from all previous arguments for indeterminism. Even those of Bergson and James, as I understand them, fail to point out this positive difference between law and variation. They find a fluent quality about facts which forever escapes the static and rigid concept. Yet one might reply to them that our concepts approach the fluent changing reality as a limit. Even though those concepts never reach that limit, they allow no irreducible remainder, which can be definitely named, to stay outside the conceptual series. The advocate of universal law may say: "You can point to no one fact which I can not come nearer and nearer to accounting for completely." The series π is never completed, yet any one term of it, which you can name, may be exactly computed. My own argument does, I think, escape this objection. It attempts to point out a well-verified character about facts which is not simply at present unexplained in detail, but would seem to be inexplicable in terms of causation, even to a perfect knowledge. The tendency to deviate, to spread out, to produce ever new sports, is indeed in substantial agreement with the Jacobean doctrine of a growing universe. But I do not think the inadequacy of any given concepts, or group of concepts, to account for motion, change, or life, can be regarded as a proof of a real spontaneity in those facts.

And the present argument goes even further. There seems to me no ground for saying that there is anything about spontaneity which is unintelligible, *i. e.*, beyond clear conception. Chance as here defined appears to be clear enough. It is a dual affair, with a collective and an individual aspect, and in my view each of these aspects is meaningless without the other. The collection is law-abiding, the individual members, within limits, ambiguous. But I do not see why ambiguity is not a perfectly clear concept. There would seem to be, then, no real reason for excluding spontaneity from the kingdom of the intellect. It should be included as a genuine scientific category, no more wonderful than law itself. Not the limitation of the understanding by something indefinable, mysterious, unaccountable, but the inclusion of that something within the sphere of clear definition, is what every thinker naturally desires.

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EXPERIMENTAL ORAL ORTHOGENICS: AN EXPERIMENTAL INVESTIGATION OF THE EFFECTS OF DENTAL TREATMENT ON MENTAL EFFICIENCY[1]

LITTLE if any attempt has hitherto been made to measure by scientific, objective means the mental improvement resulting from the correction or removal of the various physical defects which are now generally known to afflict the majority of school children. We are beginning to appreciate, from a number of recent studies, the extent of the retarding effect upon mental growth of such physical anomalies as adenoids, hypertrophied tonsils, nasal obstructions, defective ears, eyes, and mouths; but no one has attempted to deter-

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[1] Read before Section L, Education, of the American Association for the Advancement of Science, Washington, December 29, 1911.